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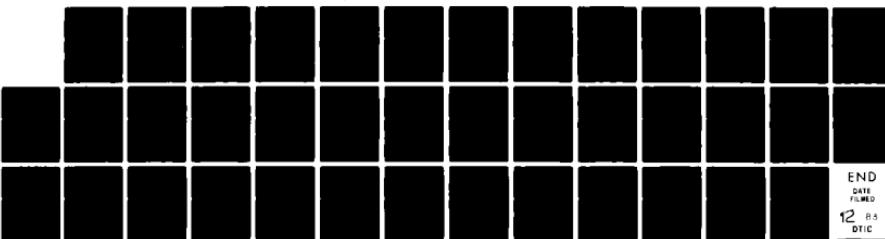
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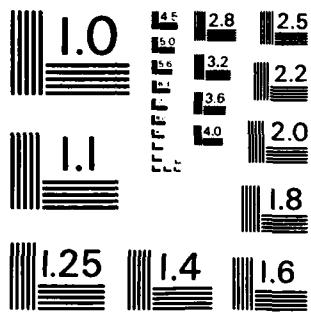
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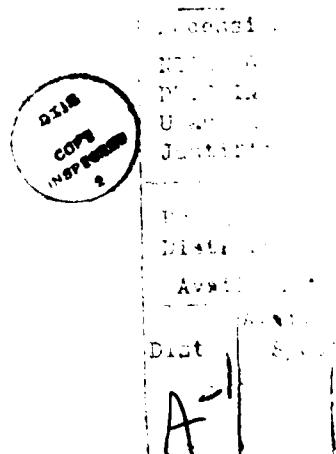
Qualitative Methods Reclaimed

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TR ONR 20
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## ABSTRACT

This epilogue to Administrative Science Quarterly's Special Issue on
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zation studies, and provides some reflections on the original issue.

## Qualitative Methods Reclaimed

### (Epilogue)

Remarks appearing in an epilogue are usually meant to tie together whatever loose ends still exist in a book by providing a general, yet final, perspective on all that has come before. Sometimes these remarks do so by detailing in a crisp, abbreviated way, events taking place after a book proper has been closed; events of the sort that presumably shed light on narrative detail. Sometimes these remarks work by revealing hidden meanings in the text, meanings a writer wants to be sure do not remain hidden for long. Sometimes an epilogue is but an epitaph that serves to commemorate, if not celebrate, the preceding words and ideas. Sometimes an epilogue is merely a convenient excuse for an author to try again to bring home some central argument or point of view. This epilogue is written in the spirit of all of these honorable, perspective seeking traditions.

Time, however, is the master perspective provider. Like goals, intentions, or purposes, perspectives are best determined retrospectively. This applies particularly well to the products of scholarship that are both long in coming and long in attracting whatever attention (if any) they receive. Certainly in academic life we write papers, deliver them more or less on time, seek out the immediate reactions and opinions of selected others (usually friendly ones), and then lay back to wait and see what will happen to our own sacred words. Four years is not a long time to wait, of course, but it is long enough to allow for another look at the papers presented here so that we might see purposes to their writing beyond those initially staked out by their authors (and editors).

A good place to begin is with the commonplace observation that the literature in organization studies is put together by those who do the

research, those who are researched, and those who read the research. Despite the variety of these groups (or perhaps because of it), this homily suggests that whatever sense a researcher is able to make of the researched is ultimately mediated by the readers of the research. They are the ones who must eventually come to terms with whatever sense is claimed in a study, for they are the ones who must put this sense to work. My first concern, then, is to report on some of the things I've heard from readers of this special issue.[1]

## Themes Uncovered

Readers have not missed the rhetorical purposes attached to the papers in this volume. There is more than a little "pontification" involved whenever our own methods are discussed since, by presenting our own ways of seeing the world as more or less definitive, irony is cast on other ways of seeing it (A. Becker, 1982). To some degree, then, these papers are part of a larger movement involving those who are attempting to rearrange and revalue the various symbols of the organization research world such that qualitative work will achieve a more prominent place than some think it now holds.[2]

Given that the rhetorical functions of social science writing are unavoidable (Gusfield, 1981: 83-108), the way in which those writing here attempt to persuade their audience also provokes reader comment. For example, noticeably absent in this volume is the use of passive constructions as a way of giving a manuscript something of a magisterial and impersonal air. The active voice is very much present in these papers, thus identifying the agent from which a given pronouncement comes. Polysyllabic and turgid prose is infrequent. While the stylistic delicacy of the crafty novelist is hardly

achieved, there is the sense among readers that the authors of these papers care as much about the presentation of their research reports as they do about the accomplishment of the research itself.

Two observations are of relevance in this regard. First, each of the papers is short, thus the social scientist's habitual use of twenty words when two will do is structurally (editorially) constrained. Second, missing from these papers are many of those vague phrases (less charitably, "bullshit qualifications") that convey, among other things, a general reluctance on an author's part to take the rap for what is being said or to specify precisely who is saying what. Examples in the existing literature are everywhere: "all things being equal, A tends to be related to B;" "it is becoming increasingly apparent ...;" "differences are found ..." These fuzzy phrases portray research results as emerging from an external world of data unsullied by the researcher's presence. They also serve as all-purpose loopholes such that if and when an author's position is questioned, the non-specificity of the writing will allow an easy out (H. Becker, 1982). The authors of the papers here seem willing, if not eager, to take the rap by offering up their generalizations (and pontifications) in ways readers find refreshingly direct.

Similarly, method is personalized in the papers of this book and readers express some enjoyment with the idiosyncracies displayed, the behind-the-scene revelations, and the mannered but distinct voices exhibited by the writers. The use of the vertical pronoun "I" shows up in these papers with remarkable frequency for an academic journal, thus documenting the fact that there really is a person behind the technique discussed. This is not to suggest that each writer should be awarded some sort of intimacy trophy for baring all the sterling and sordid, brave and cowardly detail of their respective research adventures, but it does suggest, by telling anecdote, the presence of a most human element in the research process.

Another thematic point remarked on by readers is an apparent self-consciousness on the part of the authors. Specifically, the papers in this volume drive home the fact that qualitative research is hardly a process marked by an untroubled accumulation of detached, neutral, or purely descriptive fact. Indeed, those writing in this volume would no doubt claim that any kind of research endeavor is a social and cultural process with deeply rooted moral, political, and personal overtones. Such a perspective points to a deepening sophistication within the organizational research community and helps make the researcher a visible and discussable object of scrutiny rather than a shadowy, impersonal, and hence rather feckless guide to enlightenment.

The variety of research approaches presented here also stimulates reader comment. The standard research cliche that points to the interaction of method, theory and data throws some light on this diversity. For instance, if the data are messy, disordered, or otherwise difficult to pin down, these features seem also to be reflected in the methods and theories to which such data become attached. Bonini's paradox (as formulated by Bill Starbuck and discussed by Weick, 1979) provides a good way of making this point. The paradox itself refers to an accuracy-comprehensibility tradeoff occurring when organizational simulation models are constructed and run. The more accurate, detailed, and precise a given model, the more complex, untidy, and chaotic the simulation results. In field studies a similar result is likely. The more complicated the social system studied, the more any description of that system must, to be true to the "real stuff" caught in the field, itself be complicated. Presumably, the methods selected for such study must also reflect what is to be examined. A simple method in a complex scene may well be inadequate (and vice-versa).

Consider also how method mirrors data, as well as the reverse. A near perfect example of just this is <u>Boys in White</u> (Becker, et al., 1961), the deservedly famous study of medical school education, conducted by a team of researchers of differing ages and academic status. As detailed by Hughes (1974), the research team gradually and without much planning came virtually to replicate the social structure of the institution they studied. Everett Hughes, the oldest and most prestigious member of the team, worked with the school's top administrators and senior faculty members. Howard Becker and Blanche Geer, the youngest and greenest of the researchers, took on the medical students. Anselm Strauss spent most of his time in the field among the residents and interns who occupied, as he did, the middle ranks of their respective social systems.

It is true, of course, that such mirroring effects can be disastrous when they are of a cognitive rather than structural sort. As some of the authors of this volume note with more than a little trepidation, there is always the danger that researchers will lose their uniqueness and distinctive misson and become (if only temporarily) little doctors, cops, or bureaucrats instead of the sociologists, economists, or psychologists they set out to be in the setting studied. There may be some fieldworker's pride in such a transformation, but rest assured there will also be some academic shame.

The role of surprise in research activities is yet another feature of these papers that has caught the attention of at least some readers. Unlike the Prince of Serendip stories that litter the unserious asides in many research methods textbooks, the authors here search out and, to a degree, count on finding what Agar (1983) nicely tagged "reality disjunctures." This points to the essential, if not fundamental, role incongruity plays in research of the qualitative sort.[3] In fact, a sort of general research model is visible running through the papers presented here, wherein lines of

study and theory building are occasioned by the breakdown or failure of the researcher's manifest and latent expectations. Sometimes the breakdowns are accidental ("serendipitous"), but more often they are mandated in the sense that the researcher directly seeks them out. Unexpected variance is the heart and soul of qualitative work for it brings about a conscious search for resolution. When a researcher's surprise becomes understood, theories of the studied scene (and perhaps of method as well) are enriched. To seek the exceptional is to discover the routine.[4]

Disturbing to some readers is the often maddeningly emergent or unfolding character of the research discussed in this volume. Grant-givers among others, are often furious at qualitative researchers for doing Study when they were funded to do Study A. Yet qualitative work is, by design, open-ended. It is also skeptical. Researchers in this domain are a fairly contentious lot, typically unwilling to grant much credence to another's observations or theories. Thus the perspectives and topics originally informing a proposed program of research are quite likely to shift as the research runs its course. Part of the issue is the just-mentioned central role played by incongruity and surprise in qualitative work; but another part of it concerns the largely unstandardized reporting formats that mark qualitative research papers. In the absence of conventionalized formats, the researcher must work through a lengthy chain of choices when preparing research reports for public consumption. Most of these choices are private ones that derive from the emergent nature of the project itself.

Impressions and potential themes of the research, for example, must continually be sorted out. Various interpretive schemes need to be tried for relative fit and their ability to condense and order data. Propositions and hypotheses are typically worked up and examined while the data are still being collected. At the same time, the researcher is also absorbing and culling new

ideas that may be included in the writing punctuating any investigation. The interpretations and ideas that eventually will be included in the research report come slowly into view as the writer sits down and decides what impressions are to be given voice, what analytic constructs are to be taken up, what words are to be used to illuminate them, and so on. Writing up the results of qualitative work is as much a discovery process as it is a summary of what has already been discovered (Mills, 1959). A Weickian maxim is appropriate in this regard: "Qualitative researchers often do not know exactly what they have studied until they have written it up and passed it around."[5]

Finally, readers of these papers frequently remark on what they think is the misleading label applied to the examined research methods. They point out, and I would agree, that to use the label "qualitative methods" as a descriptive device presumes the presence of an opposite and contrary set of methods with, perhaps, an equally misleading label, "quantitative methods." Whatever one is, the other is not. Yet as many readers suggest, any given researcher typically works in a number of ways. Moreover, whatever distinctive unity either camp possesses, it is a unity seen primarily by members of the other camp. As I noted in the Preface, qualitative strategies are essentially those emphasizing an interpretive approach in which data are worked with (and on), both to pose and resolve research questions. From this standpoint there is nothing hard or soft, objective or subjective about a qualitative orientation; within its reach are found a broad range of craft-like approaches to particular research problems.

## On Principle and Craft

As qualitative research unfolds, it sidesteps the hypothetico-deductive model in favor of an interpretive eclecticism designed to resolve whatever incongruities a researcher has managed to uncover in a given domain. This is

high sounding sentiment. Counterclaims exist, and certainly a portion of what passes under the qualitative banner leaves much to be desired. To the degree that qualitative researchers are a source of embarrassment to some, it is often because in the strictest sense their methods are untried, and therefore lack reputability. Professional standards, it seems, are best safeguarded (and probably enhanced) by the thoughtful articulation, encouragement, and enforcement of what is already known and practiced.

The researchers represented in this volume cannot, however, easily be dismissed for not understanding or not being familiar with conventionalized research prescriptions. In fact, almost all the writers of this book have a past history (no matter how secret or shady) of using a variety of research designs and tools. Moreover, if qualitative work is increasingly attractive to the recruits in the field, it cannot be argued that it is because they do not have the training to do anything else. Even the most lax of Ph.D. programs devoted to the training of organizational researchers typically require a semester or two of statistics, a course on research design, some exposure to philosophy of science (usually positivist), and, of course, a daily diet of the "classics" in the field.

If, as I suspect, qualitative work represents an explicit rejection of certain research models, what is being accepted by students as good practice? This is not as yet altogether clear. To move toward a definition for qualitative work is obviously difficult. Consider some of the specific research techniques linked by organizational researchers to the qualitative label: participant observation, content analysis, formal and informal interviewing, clinical case studies, life-history construction, videotaping behavioral displays, archival data surveys, historical analysis, the invention and use of varied unobtrusive measures, and various formal schools of thought and procedure in social science, such as dramaturgic analysis, semiotics,

frame analysis, ethnomethodology, and conversational analysis. Even these fairly precise technical tags are not always as telling as one might think. Participant observation, for example, really says more about the situation of the researcher than it does about what is done in a particular setting (Emerson, 1981). All forms of interviewing beg descriptions of just how the interview responses are heard, coded, and linked to theoretical interests or constructs (Cicourel, 1964). Even historical analysis often employs quite refined numerical procedures, although these activities rarely become the centerpiece of a research report (B. Bailyn, 1982).

If anything, qualitative research is marked more by a reliance on multiple sources of data than by its commitment to any one source alone. Technique-dependent definitions, then, are faulty since they cannot absorb the diversity of uses to which the qualitative label applies. It is my view that we are best off thinking of qualitative research in terms of some of the organizing principles surrounding the activities (and topics) of those who do the work. Gleaning from the articles of this volume, I have seven principles in mind (although a reader can certainly add to this list).[6]

1. Analytic Induction:  Qualitative work begins with the close-up, first hand inspection of ongoing social life. Specific and local features of the studied scene are sought as a data base from which patterns may or may not be imposed. Generalizations are to be built from the ground up, and offered tentatively on the basis of their ability to fully contain the data in hand. In the ideal, no variance remains unexplained.

2. Proximity:  Researchers place an importance on concrete occurrences and episodes, not reports of such. Investigators should witness first hand much of what is studied and, presumably, understood.[7]  To the extent possible, people should be observed engaged in activities that matter to them, the performance of which is, to them, of more importance than the fact that they are performing in front of the researcher.

- 10 -

3. Ordinary Behavior: Topics for qualitative study are located within the natural world of those studied. Qualitative research is interested in everyday activity as defined, enacted, smoothed, and made problematic by persons going about their normal routines. Whatever interrupts or otherwise disturbs or distorts ordinary lines of action is to be minimized.

4. Temporal Sensitivity: Patterns of collective behavior must be seen from both a topographical (relational) and historical perspective. Any given social action is unique and unlikely to be repeated again in precisely the same way. It may be that the more critical the event, the more rare its occurrence. Thus qualitative researchers must pay attention to historical antecedents as a way of understanding just what a given event or pattern represents, in contrast to what came before and what may come after. Such sensitivity takes many forms, from the living-in a social system over a significant amount of time to the careful survey of system-specific, historical materials, to the close inspection of broader historical records (and commentary).

5. Structure as Ritual Constraint: It is taken as axiomatic that in qualitative work given patterns of social activity are essentially arbitrary, a result of custom, present circumstance, situated motives, and ongoing interaction. From this perspective there is no primal social order or set of fundamental survival functions that exist for a collective against which some "natural deviation" might be defined. Human actions (and the organization that surrounds them) are intentional, mediated by what people think they are accomplishing. To ignore these meanings and the context within which they are contextually relevant is to impose structure rather than to discover it.

6. Descriptive Emphasis: Qualitative work involves ontological inquiry. This is merely a fancy way of saying that, at root, qualitative research seeks descriptions for what is occurring in any given place and

time. "What is going on here" is the most elementary qualitative research question, yet the most difficult to answer adequately since, typically, there are many voices and perspectives to be heard. Qualitative work, as is true for social science in general, is then essentially ironic since it aims to disclose and reveal, not merely to order and predict.

7. Shrinking Variance: Qualitative research is geared toward the discovery and explanation of similarity and coherence. Reasons behind the absence of variance are often both the subject and goal of qualitative study. Instead of asking questions about those who, in Levi-Strauss's wonderful phrase, "do things to whiten mother's hair," qualitative researchers ask, first, why it is there seem to be so few people who do whiten mother's hair. In this sense, qualitative work is concerned more with commonality and things shared in the social world than it is with differentiation and things not so shared.

These principles are, of course, quite rudimentary. They may even obscure, as much as sharpen, the image of qualitative work since to state a principle is not to say how it is used. The aim to produce a coherent description of a claim reality (and the truths it contains) may be shared by qualitative researchers, but there are, indeed, many ways such a mandate can be addressed. Fiction may be as useful as fact.[8] In his essay, "False Documents," E. L. Doctorow (1977: 219) suggests that novelists "compose false documents more valid, more real, more truthful, than the 'true' documents of the politicians or the journalists or the psychologists. Novelists know more explicitly that the world in which we live is still to be formed and that reality is amenable to any construction that is placed upon it."

Qualitative researchers would surely agree with these remarks, pointing out that Doctorow is simply saying that all descriptions purporting to be true carry with them their own interpretive standards. Here, of course, is where

qualitative research becomes a craft very much dependent on the public and private standards (aesthetic, moral, and professional) held by the researcher. Any given piece of work might violate a few or many of the principles I just enumerated, yet still be prized and valued within the qualitative research community. It seems that the most memorable and influential works in organization studies are just those that do violate standards and principles of both the qualitative and quantitative sort.

Some examples are Gouldner's Patterns of Industrial Bureaucracy (1954), Dalton's Men Who Manage (1959), and Goffman's Asylums (1961). More recently, examples are Kanter's Men and Women of the Corporation (1977), Willis's Learning to Labor (1977), and Starr's The Social Transformation of American Medicine (1982). These works defy easy classification. They wander from the numerical to the interpretive, from the production of facts to the essay, from the synthetic and theoretically derivative to the strikingly original, and so on.

The craft of these works seems to reside in the avoidance of a compulsive orthodoxy or rigid methodology. There is an apparent passion and concern for the topics of study standing behind the writing. There is an unbending commitment to authenticity and close detail, but without ducking the writer's responsibility (and authority) for establishing the meaning of such detail. Yet theory never runs away with the story in these works. Theory is, in fact, used rather sparely as a secondary theme, not a primary one. Moreover, the theory drawn on represents something of a bricolage coming from psychology, political science, sociology, economics, and so forth. To call any of these works functionalist, Marxist, interactionist, or structuralist would be to do a disservice to the many analytic traditions the authors draw from in the telling of their respective tales.

What does come through in these works is a commitment to go beyond method, to venture out beyond principles of conventional procedure or topical constraint. There is, I think, a general truth here because it seems the heroes in any field of endeavor are those who break with the principles, disobey the advice of their standard-bearing elders in the field, and question what is currently regarded as proper, smart, or useful. Most of us fail in unspectacular ways, of course, when we leave the comfortable confines of advisable practice. But not everyone fails, and those who do not become the guides for proceeding in new and exciting ways. A field grows and advances its craft to the degree that there are those on the margins looking outward, not inward, for clues on how to move ahead.[9]

At a somewhat more pedestrian level, one function of qualitative work is to enlighten a reader without disfiguring the social life described in a research report. Part of the craft of qualitative work lies, then, with the ways in which the data are handled after they have been more or less gathered. Harvesting the data of qualitative work means essentially two things: writing and rewriting. These efforts also require the soliciting of critical commentary so revisions will become more coherent and logically persuasive. This is hard work. One must be something of a statistical atheist to work well in this domain since it requires the continual forming and reforming of possible hypotheses (a practice that is anathema to the dedicated statistician). Each iteration of a research report is designed to better resolve analytic and empirical inconsistencies. This commitment to full, or at least hardy, explanation lies behind Howard Becker's (1966) oft-quoted quip about methodology being too important to be left to the methodologists. Yet even when it is not left to the methodologists, the craft of qualitative work necessarily remains partially veiled. Presumably, if we knew exactly what to do, we would go do it and stop offering advice.

For those with high status in a field, to prescribe a method is essentially to recommend powerfully what is already more or less a matter of actual experience. Such advice is most often little more than saying, "it worked for me, it'll work for you." Even the relatively loose prescriptions of the sort reprinted in this volume fail to account for the biographical particulars of potential researchers, the changing scenes in which the research takes place, the ever-shifting principles of practice that, when put to work, define the craft. This prescriptive dilemma reflects more than just my own timidity or private doubts about what it is we do. It reflects, I think, the changing basis of social science research, a change that is moving us from a rather fossilized paradigm of method to the acceptance of many methods, each of which allows paradigmatic but unique descriptions and explanations to emerge from empirical sources.

## Paradigm Lost

It is again worth reminding ourselves about the fundamental character of the sorts of things we study in organizational analysis. The kind of science possible surely depends on the kinds of things with which it must deal. In this light, consider the study of, say, plant life as it takes place in the natural sciences. Plants have a physical presence. They are there. You can turn your head away from them, look back, and they are still there. They can be measured for their stable and not-so-stable characteristics. Moreover, you can put one plant alongside another and clearly have two plants.

Now, things examined in organization studies, like industrial plants, just do not live and die predictably within some known and normal range of environmental variation. They come and go, of course, but not in any fixed or unalterable sequence. Suppose a sociologist watches these plants for a while and watches, in particular, plant closures. Two closures are seen. Has the

plant sociologist seen the same thing twice? Has anything been seen? What if this plant sociologist sees that it takes four administrators six weeks to close one plant. Will it take two administrators twelve weeks to close another plant of similar dimensions? The absurdity of these questions is the very point of this discussion. Like winks, blinks, and nods, the "thingness" of industrial plants is distinctly problematic.

In a marvelous but discomforting essay on the same theme, political scientist Etheredge (1976) points to the pluralistic and competing knowledge base underlying our studies. He poses a not-so-hypothetical problem: "The Case of the Unreturned Cafeteria Trays," wherein the manager of a college cafeteria seeks advice from behavioral scientists as to what is going on in his plant, why it is happening, and what he might do to stop it. Thirty or so different explanations are provided, along with somewhere around twenty different policies designed to get cafeteria trays back to where the manager thinks they belong. This is the structure of our knowledge.

As uncomfortable as Etheredge's little parable is, I suspect most organizational theorists have no trouble grasping the argument. There are many fragmentary theories in our field, and many imaginative (and ambitious) proponents eager to put a given theory to work. To take another somewhat more familiar example, consider the kinds of explanations that might be proposed to the question "why does Sam Stone work hard?"

> He works hard because he is given appropriate training, leadership, and direction.
>
> He works hard because he gets lots of sleep, eats well, and keeps regular hours.
>
> He works hard because his friends work hard.
>
> He works hard because, if he doesn't, he fears being fired.
>
> He works hard because his parents encouraged him to do so as a child.

He works hard because he likes what he does.

He works hard because he is well paid.

He works hard because he doesn't know any better.

Almost instantly a number of quite different answers can be generated to this rather straightforward inquiry. All are valid responses since they address the "why" questions with the proper and respected "because" answer. More to the point, we can easily imagine situations that would make any particular "because" a cogent and compelling response. This is a simple way of noting that ours is not only a field where multiple theories can coexist with a degree of impunity, but also a highly context-sensitive field.

The "paradigm lost" to which this section title refers is essentially the experimental, positivist, and empirical correspondence paradigm of the natural and physical sciences. It is the prescription to generate propositional statements first, and then test them by reference to observable fact. The inextricable dilemmas surrounding this approach are fairly well documented, but two problems are worth quick restatement.[10]

The first problem of the paradigm is that the required independence of theory, fact, and method simply will not hold for the social sciences (if it will hold anywhere). Like Sam Stone's hard work, social actions can be read in multiple ways. The responses of a worker to a questionnaire on job performance, for instance, can be viewed by the plant sociologist as an informant report on specified features of a given organization. The same responses can also be viewed by the ethnomethodologist as evidence about the routine properties of form completion, an everyday task more or less routinized and therefore itself a subject of analysis. The data are given meaning by a researcher, then, only by reference to the theory to which they are made relevant.

The second problem of the paradigm is that suggested by Popper (1963) who wondered mightily about where given theories originate. If the simple-minded illustrations used here can generate a lengthy set of plausible propositions, how does one go about selecting from among them those most worthy of empirical test? Science won't help us here. Consider Pirsig's (1974) delightful little novel, Zen and the Art of Motorcycle Maintenance, in which the protagonist abandons science when he realizes that science always leads to the development of far more theoretical questions than can possibly be tested or solved. This is not to suggest that we abandon our pursuits as some readers felt the frontispiece ("The Seminar") of this volume recommends. It is to suggest, however, that we undertake our studies with a degree of appropriate modesty and restraint.

Perhaps some of this modesty and restraint can be had by viewing our theories as mere similes, able to catch a part of the object of study, but only a part. Take my favorite simile of "impression management," or "life as theater," as analyzed by Messenger, Sampson and Towne (1962). Such a simile treats naturally occurring episodes of social interaction "as if" such episodes were occurring on stage. Dramaturgic analysis then becomes useful. Thus props, plots, and plops are sought out, rehearsals and opening nights discovered, backstage and frontstage regions plotted out, faulty performances as well as splendid ones critiqued, instances of stagefright and overacting catalogued, and so forth. This is a particularly potent simile, but still it captures and analyzes only a few of the purposes to which a given interaction might be put. People may indeed wish to manage their impressions in order to make good ones, but they may also wish from their interactions to make money, make time, make cars, make haste, make change, make out, make children, make clear, or make fun.

If treating theory as simile helps inform qualitative work, we are still faced with Popper's problem, where do we go for good ones? Here is where I think we must go directly to the social world itself and attempt to uncover what people are up to at any given time in differing contexts. Moreover, we must be careful, in so doing, not to bring too many purposes beyond the descriptive with us, lest some of the distinct advantages of qualitative work be lost. If, for example, a man rests his head on a chopping block expecting to get a hair cut, the fact that his head drops off is not of great interest to the qualitative researcher. Losing one's head under such circumstances is mere behavior, it is not social conduct (White, 1976). This is to say the questions surrounding the consequences of behavior may be very different than the questions surrounding the nature of human conduct. It seems to me the study of conduct or social action will remain the special province of qualitative research since it necessitates an interpretive approach. The study of behavioral consequence is also important, whether or not we aim to design efficient barber shops or guillotines. But to emphasize consequence is to bypass, by omission or intention, the ontological question of "what is going on."

In looking about for interesting theory, it is obvious that we do not have any shortage in organizational studies. There are certainly more than enough around to keep us busy testing and elaborating well into the next century. This is not the only path to be followed. If my sense of paradigm lost is accurate, we must not expect great theories to rise and fall again in the social sciences. We are now far too segmented, specialized, suspicious, and savage to allow any one theory group to dominate the field. Theory building will continue of course, but given what I suspect is a general disenchantment with social theory of all sorts, those theories that are constructed will most likely be used selectively as similes to order data in a

particular and fairly narrow domain. To hold out for the grand paradigm in the human sciences is akin to being a member of some cargo cult.

## The Final Word(s)

To conclude, a cautionary note. Time seems always to erode purpose and highlight form. Method discussions, such as this one, also erode purpose. The danger of pushing method is that it may become an end in itself. This is as true for those in the self-proclaimed empathetic and humanistic enclaves of qualitative research as it is for those merry pranksters of the social psychology laboratory, those diabolic taxonomists of the population ecology schools, or even those practical and zealous theorists on missions to save organizations. One doesn't have to be the crudely (perhaps cruelly) stereotyped number-crunching, model-building, theory tester to allow form to overcome function. If Woody Allen is right in his claim that ninety nine point nine percent of success lies in getting it done and turned in on time, we must not make an obsession of our methods. To get on with our various research programs is the thing of foremost concern. If we can do so with a bit more appreciation for the diversity of these programs, then perhaps the detour through this land of many methods will have been worthwhile.

## Notes

1. As always, the methods used to build a particular description deserve scrutiny. In this case, they don't deserve much. What I have done in the following section is merely to organize thematically some of the comments I have heard about this special issue over the past few years. There is nothing systematic about my hearing except those sociological biases that are mobilized around my specific place in space and time, and those not-so-very-peculiar psychological biases that probably make me a better hearer of the celebratory and laudatory comments coming from readers than those of a defiling and critical kind. At some time or other I have used all of the writings in ـhis volume as fodder for graduate students in research seminars and, as a result, have been, perhaps, overimpressed by what recruits to the field have had to say rather than their sage elders.

2. Since the special issue was published in December of 1979, the attention provided qualitative methods seems not to have waned. If anything, there is greater interest today than there was then. Emerson (1983) has a new fieldwork reader on the market. Spradley (1979, 1980) has two method texts available (regrettably, posthumous). Agar (1980) has another. Webb, et al. (1981) have released a second version of their unobtrusive measures book, this time called "non reactive" measures. Of more direct relevance, two of the six monographs published in the Sage series on "Studying Organizations" deal with qualitative methods (McGrath, Martin and Kulka, 1982; Van Maanen, Dabbs and Faulkner, 1982).

3. The principle of incongruity as fundamental to the awareness process was, perhaps, best addressed by Burke (1965). There are, of course, importantphilosophical distinctions to be made concerning such a process (e.g., Schutz, 1970; Gadamer, 1975). If, however, readers feel philosophy is important enough to be left to the philosophers, Glaser and Strauss (1967) put forth a discovery technique of general value. of more recent vintage, I find the theories of description provided by Agar (1982, 1983) most useful.

4. I have developed this point somewhat more systematically elsewhere (Van Maanen, 1979). The theory on which it rests and the methodological implications that follow are best described in Becker (1970).

5. Such retrospection may be partially responsible for the repeated insertion of substantive findings in the papers of this volume. Method and result are closely intertwined in qualitative work, and it is difficult to discuss one without considering the other. Readers seem to appreciate this linking of substance and method, because it represents something of a break from much of the prescriptive writings that ritualize data collection, institutionalize analytic technique by isolating it from fresh data, and sterilize research reporting by

linking it to a fixed format. If nothing else, the writers here push
for a more playful, experimental approach to the doing and reporting of
organizational research. The best treatment on the cognitive aspects of
all phases of social research and on the merging of analysis and
findings in the reporting of empirical work is L. Bailyn (1977).

6. Some of these "principles" are put forth in Van Maanen, Dabbs and
Faulkner (1982): 16-17). That I am easily able to add to the list (or
take away from it) suggests, as I do later in the text, that there is
nothing holy being discussed here.

7. It is worth noting that this principle urges the principal investigator
to go into the field and collect data. there is no substitute for
first-hand inspection in qualitative work. The real thing is not to be
found in other people's data, even if these other people are hired hands
(and maybe especially then). This runs counter to custom in the
organizational research tribe. Schwartz and Jacobs (1979: 310)
suggested an inverse relationship between the collecting of data and the
fame and reputation of the researcher. In many quantitative studies,
for example, anonymous graduate student "workers" catch and run the data
while the mane brand "professional theorists" analyze it. This is
thought to be good training for the recruits, and efficient use of time
for the veterans who, after all, "already know what is happening out
there." most disturbing in light of the proximity principle is,
however, the fact that it is quite possible to get a Ph.D. in any of the
fields concerned with organizational study without ever observing
organizational life in any detail, up close, for any length of time.

8. Krieger (1983: 173-199) had some wonderful things to say about the role
of fiction in social science. Among the intriguing points raised in her
brief essay is the view that current social science does not allow many
dimensions of the outside world to enter into its discourse. While our
methods are supposed to prevent this, Krieger suggests that our
insulation stems from allowing our evidence to follow our arguments,
rather than the reverse.

9. This matter of craft bears further attention, for it cuts to the core of
the purpose of this volume: to promote worthy, qualitative studies of
organizations. As Hughes (1958) suggested, in virtually every sphere of
vocational competence people are judged by peers using standards that go
well beyond whatever minimal levels of skill are required to meet basic
tasks. That these standards cannot be fully formulated in advance of
performance does not seem to bother, for example, most medical doctors,
garage mechanics, teachers, clerics, policemen, fishermen, or social
workers. Such standards, in general, seem to form around the ability of
practitioners to meet and master the unexpected. In short, craft calls
for the exercise of judgment and skill when one is faced with puzzling
and pressing matters. To say we cannot prepare organizational
researchers for tasks that have no names is altogether specious, since
other occupations do so by deliberately preparing would-be members to
seek out and come to terms with the unexpected, the dramatic, the
anomalous. Parachuting graduate students into organizational fields
from which they are expected to bring back qualitative field reports
might be one way to begin such preparation.

10. See, for example, Burrell and Morgan (1979) on the meaning of multiple paradigms in organizational analysis.One possible line of resolution consistent with the empirical and more or less positivist traditions of our field is the Lakatosian approach to theory validation which, in light of current practices, makes relatively little use of the rack of nature. Theories win out only in context-specified ways. Facts are suspect in such a research approach because validation rests on theory-to-theory comparisons, rather than theory-to-fact comparisons (Lakatos, 1970).

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